

liarities of the principal plants in each order are briefly described as well as their economic uses, together with notes on the most general method of propagation.

The book will no doubt meet with a wide circulation; the chapters on propagation, grafting, and budding, being of a practical character, will be useful to other readers besides those of a purely scientific turn. As a further illustration of this we may point to the chapter on Seed-saving, in which we are reminded of the excellence of the produce of the Continental seed farms, especially those of Erfurt, which are noted for their Primulas, Stocks, Balsams, Asters, &c.; we are also told that the seed of such common plants as Cineraria and Calceolaria is, when of a "good strain," worth from 10*l.* to 15*l.* per ounce, and Primula seeds even more. The book has a good index, always a special point of value in one intended for reference.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

The "Hibernation" of Birds

I TRUST your correspondent "X" will not object to my pointing out that the "hibernation" story which he retails in NATURE (vol. xvi., p. 43) has in common with dozens, not to say scores, of others, the defect of being delivered at second-hand, without even the slightly confirmatory evidence which the name of the observer of the marvel and of the place where it occurred would supply. As such it may surely be dismissed to the limbo of (I will say) legends. That which has been furnished by the Duke of Argyll (vol. xv., p. 528) rests on a better base, and is almost the first I have met with for which a respectable man vouches. Far be it from me to deny the possibility of a miracle being wrought in Persia, but I confess that without impugning Sir John McNeill's veracity, I simply refuse to believe the story except I regard as miraculous the incident he relates. Now there is a common supposition that miracles are only performed with some worthy end in view, and, moreover, that unless a miracle is recognised as such by the witnesses thereto its effect is nullified. This supposition may, however, be wrong, for it is hard to conceive what may have been the end of this miracle, and its supernatural quality is clearly not acknowledged by the distinguished persons present. One would find greater satisfaction, perhaps, if the Duke of Argyll had expressed his belief in it, but he contents himself with saying that he had "frequently heard" his brother-in-law relate the circumstance. The identity of this phrase with that said to have been used by another great man on another occasion is very striking, and since the story, though old, is short, perhaps you will let me tell it. I have heard that King George IV., some years before his death, was strongly possessed with the notion that he had been present at the battle of Waterloo. Once as he was recounting his personal adventures on that eventful day, he perceived some uncourtly sign of incredulity shown by one of his audience. Turning to the late Duke of Wellington, who was of the company, he appealed to him—"Isn't that true, Arthur?" The adroit reply was—"I have frequently heard your Majesty say so."

May 19

THE REVIEWER OF PALMÉN'S WORK

Barlow's and Laslett's Determination of the Elasticity and Strength of Timber

ON comparing the results of Mr. Barlow's determination of the modulus of elasticity of Teak timber, given in his "Strength of Materials," p. 82, sixth edition, with the results of different Dutch investigators on Djati timber of Java, I was struck by the great difference between those results, those of Mr. Barlow being very much higher than the others, though Teak of British India and Javanese Djati are merely different names for the same species, *Tectona grandis*, L., of the family Verbenaceæ. My own experiments, which will, I hope, be afterwards published

in my inaugural dissertation, show also the complete identity between Teak and Djati. This led me to detect a serious error in Mr. Barlow's calculations. He measures the deflection produced by a certain weight of a batten 7 feet by 2 inches square, supported at both ends on two props, the bearing distance being 6 feet, as is stated on p. 67 in the general description of his arrangements. The value of E is found in this case by the formula—

$$E = \frac{l^3 w}{16 ad^3 \delta}$$

in which l is the bearing distance; in the case of Mr. Barlow, equal to 6 feet. Now all Mr. Barlow's E 's are calculated by introducing $l = 7$ feet instead of $l = 6$ feet, as should be; the consequence is that all those values are too great. So for Teak timber the value of E is found to be = 603,600 lbs., while the true value is $E = 380,023$ lbs. on the square inch. Also in the formula for the strength—

$$S = \frac{l w}{4 ad^2}$$

l is the bearing distance, Mr. Barlow calculates $S = 2,462$, taking $l = 7$ feet, the real value being $S = 2,110.5$ for $l = 6$ feet.

Mr. Thomas Laslett, in his "Timber and Timber-trees" (London: Macmillan and Co., 1875), following the arrangements of Mr. Barlow, commits the same error. On p. 42 he tells us that in all his experiments pieces were taken 2" x 2" x 84" = 336 cubic inches, and that each piece was placed upon supports exactly 6 feet apart. But for l is taken, instead of the bearing distance 6 feet, the whole length 7 feet. So all the numbers for the moduli of elasticity of the different woods calculated by Mr. Laslett are too great in the proportion $7^3 : 6^3$, and the numbers for the strength in the proportion 7 : 6.

The reduced values for E from Mr. Laslett, namely, $E = 362,870$ and $E = 305,876$, and that from Mr. Barlow, $E = 380,023$, agree tolerably well with the mean results of Dutch investigators, $E = 404,210$, and much better than does the uncorrected value, $E = 555,180$. Other determinations of the E of Teak timber are not known to me.

The results of Mr. Barlow were already published in the year 1817. Since that time several editions of this valuable work have appeared; in the year 1867 the sixth edition, revised by his two sons. General Morin gives in his "Résistance des Matériaux" all the results of Mr. Barlow on timber, reduced to metric weights and measures. Also MM. Chevandier and Wertheim, in their "Mémoire sur les Propriétés Mécaniques du Bois." It is scarcely to be believed that none of these eminent men, nor any one else, have remarked this error in the calculation of Mr. Barlow's often used numbers.

S. FIGEE

Haarlem, Holland

Basking Shark

I THINK it but just to Prof. Bocage to ask you to publish the inclosed letter, which only reached me on the first of this month, owing to its having been sent to a wrong address. I regret that I overlooked Signor Capello's memoir on *Selache maxima*, which was so plainly indicated in the *Zoological Record* for 1869. I had, indeed, the "Catalogo dos peixes de Portugal que existem no Museu de Lisboa, por F. de Brito Capello," which was published in No. vi. of the *Lisbon Journal of Science*; but No. vii., which the author (whose kindnesses to me when at Lisbon, in 1868, I cannot forget) sent to me, I never got, and hence one cause of my oversight.

E. PERCEVAL WRIGHT

Trinity College, Dublin

Lisbonne, le 14 février, 1877

MONSIEUR ET HONORÉ CONFRÈRE,—Dans l'article que vous avez publié dans la NATURE sur le "Basking Shark," vous avez, comme le Professeur Paul Gervais et d'autres, attribué à M. Steenstrup la découverte des appareils tamisants ou fanons branchiaux du squalé pèlerin ou *Squalus maximus*. Si vous vous donnez la peine de consulter No. vii. du *Journal des Sciences Math., Phys. et Nat. de Lisbonne*, vous y trouverez, à p. 236, la description de cet appareil; vous trouverez également ces appareils figurés sur la planche qui accompagne cet article et qui contient aussi la figure du poisson. La description et la figure des appareils branchiaux ou des fanons branchiaux du *Sq. maximus* (et des espèces congénères) ont été donc publiés par M. Capello, aide-naturaliste au Muséum de Lisbonne, en août de 1869; c'est-à-dire 4 ans avant la publication de l'article

de M. Steenstrup. Si vous consultez le *Record of Zoological Literature* pour 1869 (vol. vi.), vous y trouverez, p. 139 :—

"*Selache maxima*.—A detailed description and figure of an example from the coast of Portugal is given by M. Capello under the name of *Cetorhinus blainvillii*, *Four. Ac. Sc. Lisbon*, No. vii., p. 233."

Je vous envoie par la poste le No. vii. du *Four. des Sc. de Lisbonne*.
J. N. BARBOSA BOCAGE

Gold in Carboniferous Conglomerate

MANY of your readers are aware that the fact of the occurrence of gold in Lower Carboniferous conglomerate as in New South Wales is not at all new. The Gay's River Gold Field of Nova Scotia, where the gold occurs in Lower Carboniferous conglomerate resting on the edges of Cambrian slates having small veins of auriferous quartz, was first pointed out by Prof. Hart and elaborated by myself in a paper communicated to the Nova Scotian Institute of Natural Science in 1866. In Dawson's "Accadian Geology," of 1868, the same fact is referred to; also in Selaria, Prof. R. Jones received specimens of the conglomerate from me in Paris, 1867, to satisfy Sir R. J. Murchison of the fact. In the collection of ores and concrete minerals sent by H. S. Poole, Esq., Government Inspector of Mines to the Centennial Exhibition, in my charge, was a very instructive specimen of slate with a little of the conglomerate attached, having a beautiful display of gold. This was much admired. The conglomerate of Gay's River is overlaid by limestone with Lower Carboniferous fauna and gypsums. The conglomerate is worked still with good results.

D. HONEYMAN

Provincial Museum, Halifax, Nova Scotia

Japanese Mirrors

A SHORT time ago a friend showed me a curious effect, which I had previously heard of, but had never seen. The ladies of Japan use, in making their toilet, a small round mirror about $\frac{1}{2}$ to $\frac{3}{4}$ inch in thickness, made of a kind of speculum metal, brightly polished and coated with mercury. At the back there are usually various devices, Japanese or Chinese written characters, badges, &c., standing in strong relief, and brightly polished like the front surface. Now if the direct rays of the sun are allowed to fall upon the front of the mirror and are then reflected on to a screen, in a great many cases, though not in all, the figures at the back will appear to shine through the substance of the mirror as bright lines upon a moderately bright ground.

I have since tried several mirrors as sold in the shops, and in most cases the appearance described has been observed with more or less distinctness.

I have been unable to find a satisfactory explanation of this fact, but on considering the mode of manufacture I was led to suppose that the pressure to which the mirror was subjected during polishing, and which is greatest on the parts in relief, was concerned in the production of the figures. On putting this to the test by rubbing the back of the mirror with a blunt pointed instrument, and permitting the rays of the sun to be reflected from the front surface, a bright line appeared in the image corresponding to the position of the part rubbed. This experiment is quite easy to repeat, a scratch with a knife or with any other hard body is sufficient. It would seem as if the pressure upon the back during polishing caused some change in the reflecting surface corresponding to the raised parts whereby the amount of light reflected was greater; or supposing that of the light which falls upon the surface, a part is absorbed and the rest reflected, those parts corresponding to the raised portions on the back are altered by the pressure in such a way that less is absorbed, and therefore a bright image appears. This, of course, is not an explanation of the phenomenon, but I put it forward as perhaps indicating the direction in which a true explanation may be looked for.

The following account of the manufacture of the Japanese mirrors is taken from a paper by Dr. Geerts, read before the Asiatic Society of Japan, and appearing in their *Transactions* for 1875-76, p. 39 :—

"For preparing the mould, which consists of two halves, put together with their concave surfaces, the workman first powders a kind of rough plastic clay, and mixes this with levigated powder of a blackish "tuff-stone" and a little charcoal powder and water, till the paste is plastic and suitable for being moulded. It is then roughly formed by the aid of a wooden frame into square or round cakes; the surface of the latter is covered with a levi-

gated half-liquid mixture of powdered "chamotte" (old crucibles which have served for melting bronze or copper) and water. Thus well prepared, the blackish paste in the frame receives the concave designs by the aid of woodcuts, cut in relief. The two halves of the mould are put together in the frame and dried. Several of these flat moulds are then placed in a melting box made of clay and "chamotte." This box has on the top an opening, into which the liquid bronze is poured, after it has been melted in small fire-proof clay crucibles. The liquid metal naturally fills all openings inside the box, and consequently also the cavities of the moulds. For mirrors of first quality the following metal mixture is used in one of the largest mirror foundries in Kiôto :—

Lead	...	...	...	...	5 parts.
Tin	...	...	...	...	15 "
Copper	...	...	...	...	80 "
					100

For mirrors of inferior quality is taken—

Lead	...	...	...	...	10 parts.
Natural sulphide of lead and antimony	...	...	...	...	10 "
Copper	...	...	...	...	80 "
					100

"After being cooled the melting-box and moulds are crushed and the mirrors taken away. These are then cut, scoured, and filed until the mirror is roughly finished. They are then first polished with a polishing powder called *to-no-ki*, which consists of the levigated powder of a soft kind of whetstone (*to-ishi*) found in Yamato and many other places. Secondly, the mirrors are polished with a piece of charcoal and water, the charcoal of the wood, *ho-no-ki* (*Magnolia hypoleuca*) being preferred as the best for this purpose. When the surface of the mirror is well polished it is covered with a layer of mercury amalgam, consisting of quicksilver, tin, and a little lead. The amalgam is rubbed vigorously with a piece of soft leather, which manipulation must be continued for a long time until the excess of mercury is expelled and the mirror has got a fine, bright, reflecting surface."

R. W. ATKINSON

University of Tokio, Japan

THE DECENNIAL PERIOD OF MAGNETIC VARIATIONS, AND OF SUN-SPOT FREQUENCY

A CENTURY and half ago Graham discovered that the north end of a magnetic needle moved from morning till afternoon towards the west, returning thereafter to its most easterly position in the morning again. Van Swinden, who, half a century later, studied this phenomenon during several years, occupied himself greatly with the deviations from the diurnal law. One of these, the occurrence of the greatest westerly position before noon or after 4 P.M., he found to happen most frequently in 1776, the number of times increasing from 1772, and diminishing from the year of maximum till 1780. He then asked the question whether there was not a period of *eight years*. Van Swinden's results were greatly affected by imperfections of his instrument, and we can only consider that the excess of irregular days in 1776 was probably chiefly due to real causes.

Though several series of magnetic observations were made during the eighteenth century, and two series early in this (those of Beaufoy and Arago), yet, as far as I can discover, Kaemtz seems (in 1836) to have been the first to remark that the mean value of the diurnal oscillation of the magnetic needle was not constant, but varied from year to year: this conclusion he founded on Cassini's observations, which gave the mean oscillation 9° 71 in 1784, and 15° 10 in 1787. The illustrious Gauss drew more distinct attention to the fact, for, in studying the observations made at Göttingen in the years 1834 to 1837, he pointed out that the mean diurnal oscillation for each month in the second year was greater than that for the corresponding month of the first year; and that a similar increase was to be found in the third year compared with